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THE NATURE OF HEAT[1]

I PROPOSE to consider on the present occasion some of our fundamental ideas with regard to the nature of heat, and in particular to suggest that we might with advantage import into our modern theory some of the ideas of the old caloric or material theory which has for so long a time been forgotten and discredited. In so doing I may appear to many of you to be taking a retrograde step, because the caloric theory is generally represented as being fundamentally opposed to the kinetic theory and to the law of the conservation of energy. I would, therefore, remark at the outset that this is not necessarily the case, provided that the theory is rightly interpreted and applied in accordance with experiment. Mistakes have been made on both theories, but the method commonly adopted of selecting all the mistakes made in the application of the caloric theory and contrasting them with the correct deductions from the kinetic theory has created an erroneous impression that there is something fundamentally wrong about the caloric theory, and that it is in the nature of things incapable of correctly representing the facts. I shall endeavor to show that this fictitious antagonism between the two theories is without real foundation. They should rather be regarded as different ways of describing the same phenomena. Neither is complete without the other. The kinetic theory is generally preferable for elementary exposition, and has come to

[1] Address of the president to the Mathematical and Physical Science Section of the British Association for the Advancement of Science. Dundee, 1912. The introductory remarks have been omitted.

be almost exclusively adopted for this purpose; but in many cases the caloric theory would have the advantage of emphasizing at the outset the importance of fundamental facts which are too often obscured in the prevailing method of treatment.

The explanation of the development of heat by friction was one of the earliest difficulties encountered by the caloric theory. One explanation, maintained by Cavendish and others, was simply that caloric was generated *de novo* by friction in much the same way as electricity. Another explanation, more commonly adopted, was that the fragments of solid, abraded in such operations as boring cannon, had a smaller capacity for heat than the original material. Caloric already existing in the substance was regarded as being squeezed or ground out of it without any fresh caloric being actually generated. The probability of the second explanation was negatived by the celebrated experiments of Rumford and Davy; who concluded that friction did not diminish the capacities of bodies for heat, and that it could not be a material substance because the supply obtainable by friction appeared to be inexhaustible. Rumford also showed that no increase of weight in a body when heated could be detected by the most delicate apparatus available in his time. Caloric evidently did not possess to any marked extent the properties of an ordinary ponderable fluid; but, if it had any real existence and was not merely a convenient mathematical fiction, it must be something of the same nature as the electric fluids, which had already played so useful a part in the description of phenomena, although their actual existence as physical entities had not then been demonstrated. Heat, as Rumford and Davy maintained, might be merely a mode of motion or a vibration of the ultimate particles of matter, but the idea in this form was too vague to serve as a basis of measurement or calculation. The simple conception of caloric, as a measurable quantity of something, sufficed for many purposes, and led in the hands of Laplace and others to correct results for the ratio of the specific heats, the adiabatic equation of gases, and other fundamental points of theory, though many problems in the relations of heat and work remained obscure.

The greatest contribution of the caloric theory to thermodynamics was the production of Carnot's immortal "Reflections on the Motive Power of Heat." It is one of the most remarkable illustrations of the undeserved discredit into which the caloric theory has fallen, that this work, the very foundation of modern thermodynamics, should still be misrepresented, and its logic assailed, on the ground that much of the reasoning is expressed in the language of the caloric theory. In justice to Carnot, even at the risk of wearying you with an oft-told tale, I can not refrain from taking this opportunity of reviewing the essential points of his reasoning, because it affords incidentally the best introduction to the conception of caloric, and explains how a quantity of caloric is to be measured.

At the time when Carnot wrote, the industrial importance of the steam engine was already established, and the economy gained by expansive working was generally appreciated. The air-engine, and a primitive form of the internal-combustion engine, had recently been invented. On account of the high value of the latent heat of steam, it was confidently expected that more work might be obtained from a given quantity of heat or fuel by employing some other working substance, such as alcohol or ether, in place of steam. Carnot set himself to investigate the conditions under which motive-power was obtainable

from heat, how the efficiency was limited, and whether other agents were preferable to steam. These were questions of immediate practical importance to the engineer, but the answer which Carnot found embraces the whole range of science in its ever widening scope.

In discussing the production of work from heat it is necessary, as Carnot points out, to consider a complete series or cycle of operations in which the working substance, and all parts of the engine are restored on completion of the cycle to their initial state. Nothing but heat, or its equivalent fuel, may be supplied to the engine. Otherwise part of the motive power obtained might be due, not to heat alone, but to some change in the working substance, or in the disposition of the mechanism. Carnot here assumes the fundamental axiom of the cycle, which he states as follows: "*When a body has undergone any changes, and, after a certain number of transformations, is brought back identically to its original state, considered relatively to density, temperature, and mode of aggregation, it must contain the same quantity of heat as it contained originally.*" This does not limit the practical application of the theory, because all machines repeat a regular series of operations, which may be reduced in theory to an equivalent cycle in which everything is restored to its initial state.

The most essential feature of the working of all heat-engines, considered apart from details of mechanism, is the production of motive power by alternate expansion or contraction, or heating and cooling of the working substance. This necessitates the existence of a difference of temperature, produced by combustion or otherwise, between two bodies, such as the boiler and condenser of a steam engine, which may be regarded as the source and sink of heat respectively. Wherever a difference of temperature exists, it may be made a source of motive power, and conversely without difference of temperature, no motive power can be obtained from heat by a cyclical or continuous process. From this consideration Carnot deduces the simple and sufficient rule for obtaining the maximum effect: "*In order to realize the maximum effect, it is necessary that, in the process employed, there should not be any direct interchange of heat between bodies at sensibly different temperatures.*" Direct transference of heat between bodies at sensibly different temperatures would be equivalent to wasting a difference of temperature which might have been utilized for the production of motive power. Equality of temperature is here assumed as the limiting condition of thermal equilibrium, such that an infinitesimal difference of temperature will suffice to determine the flow of heat in either direction. An engine satisfying Carnot's rule will be reversible so far as the thermal operations are concerned. Carnot makes use of this property of reversibility in deducing his formal proof that an engine of this type possesses the maximum efficiency. If in the usual or direct method of working such an engine takes a quantity of heat Q from the source, rejects heat to the condenser, and gives a balance of useful work W per cycle, when the engine is reversed and supplied with motive power W per cycle it will in the limit take the same quantity of heat from the condenser as it previously rejected, and return to the source the same quantity of heat Q as it took from it when working direct. All such engines must have the same efficiency (measured by the ratio W/Q of the work done to the heat taken from the source) whatever the working substance, provided that they work between the same temperature limits. For, if this

were not the case, it would be theoretically possible, by employing the most efficient to drive the least efficient reversible engine backwards, to restore to the source all the heat taken from it, and to obtain a balance of useful work without the consumption of fuel; a result sufficiently improbable to serve as the basis of a formal proof. Carnot thus deduces his famous principle, which he states as follows: "*The motive power obtainable from heat is independent of the agents set at work to realize it. Its quantity is fixed solely by the temperatures between which in the limit the transfer of heat takes place.*"

Objection is commonly taken to Carnot's proof, on the ground that the combination which he imagines might produce a balance of useful work without infringing the principle of conservation of energy, or constituting what we now understand as perpetual motion of the ordinary kind in mechanics. It has become the fashion to introduce the conservation of energy in the course of the proof, and to make a final appeal to some additional axiom. Any proof of this kind must always be to some extent a matter of taste; but since Carnot's principle can not be deduced from the conservation of energy alone, it seems a pity to complicate the proof by appealing to it. For the particular object in view, the absurdity of a heat engine working without fuel appears to afford the most appropriate improbability which could be invoked. The final appeal must be to experiment in any case. At the present time the experimental verification of Carnot's principle in its widest application so far outweighs the validity of any deductive proof, that we might well rest content with the logic that satisfied Carnot instead of confusing the issue by disputing his reasoning.

Carnot himself proceeded to test his principle in every possible way by comparison with experiment as far as the scanty data available in his time would permit. He also made several important deductions from it, which were contrary to received opinion at the time, but have since been accurately verified. He appears to have worked out these results analytically in the first instance, as indicated by his footnotes, and to have translated his equations into words in the text for the benefit of his non-mathematical readers. In consequence of this, some of the most important conclusions appear to have been overlooked or attributed to others. Owing to want of exact knowledge of the properties of substances over extended ranges of temperature, he was unable to apply his principle directly in the general form for any temperature limits. We still labor to a less extent under the same disability at the present day. He showed, however, that a great simplification was effected in its application by considering a cycle of infinitesimal range at any temperature t. In this simple case the principle is equivalent to the assertion that the work obtainable from a unit of heat per degree fall (or per degree range of the cycle) at a temperature t, is some function $F't$ of the temperature (generally known as Carnot's function), which must be the same for all substances at the same temperature. From the rough data then available for the properties of steam, alcohol and air, he was able to calculate the numerical values of this function in kilogrammeters of work per kilocalorie of heat at various temperatures between 0° and 100° C., and to show that it was probably the same for different substances at the same temperature within the limits of experimental error. For the vapor of alcohol at its boiling point, 78°.7 C., he found the value $F't = 1.230$ kilogrammeter per kilocalorie per degree fall. For

steam at the same temperature he found nearly the same value, namely, $F't = 1.212$. Thus no advantage in point of efficiency could be gained by employing the vapor of alcohol in place of steam. He was also able to show that the work obtainable from a kilocalorie per degree fall probably diminished with rise of temperature, but his data were not sufficiently exact to indicate the law of the variation.

The equation which Carnot employed in deducing the numerical values of his function from the experimental data for steam and alcohol is simply the direct expression of his principle as applied to a saturated vapor. It is now generally known as Clapeyron's equation, because Carnot did not happen to give the equation itself in algebraic form, although the principle and details of the calculation were most minutely and accurately described. In calculating the value of his function for air, Carnot made use of the known value of the difference of the specific heats at constant pressure and volume. He showed that this difference must be the same for equal volumes of all gases measured under the same temperature and pressure, whereas it had always previously been assumed that the ratio (not the difference) of the specific heats was the same for different gases. He also gave a general expression for the heat absorbed by a gas in expanding at constant temperature, and showed that it must bear a constant ratio to the work of expansion. These results were verified experimentally some years later, in part by Dulong, and more completely by Joule, but Carnot's theoretical prediction has generally been overlooked, although it was of the greatest interest and importance. The reason of this neglect is probably to be found in the fact that Carnot's expressions contained the unknown function $F't$ of the temperature, the form of which could not be deduced without making some assumptions with regard to the nature of heat and the scale on which temperature should be measured.

It was my privilege to discover a few years ago that Carnot himself had actually given the correct solution of this fundamental problem in one of his most important footnotes, where it had lain buried and unnoticed for more than eighty years. He showed by a most direct application of the caloric theory, that if temperature was measured on the scale of a perfect gas (which is now universally adopted) the value of his function $F't$ on the caloric theory would be the same at all temperatures, and might be represented simply by a numerical constant A (our "mechanical equivalent") depending on the units adopted for work and heat. In other words, the work W done by a quantity of caloric Q in a Carnot cycle of range T to T_0 on the gas scale would be represented by the simple equation:

$$W = AQ(T - T_0).$$

It is at once obvious that this solution, obtained by Carnot from the caloric theory, so far from being inconsistent with the mechanical theory of heat, is a direct statement of the law of conservation of energy as applied to the Carnot cycle. If the lower limit T_0 of the cycle is taken at the absolute zero of the gas thermometer, we observe that the maximum quantity of work obtainable from a quantity of caloric Q at a temperature T is simply AQT, which represents the absolute value of the energy carried by the caloric taken from the source at the temperature T. The energy of the caloric rejected at the temperature T_0 is AQT_0. The external work done is equal to the difference between the quantities of heat energy supplied and rejected in the cycle.

The analogy which Carnot himself em-

ployed in the interpretation of this equation was the oft-quoted analogy of the waterfall. Caloric might be regarded as possessing motive power or energy in virtue of elevation of temperature just as water may be said to possess motive power in virtue of its head or pressure. The limit of motive power obtainable by a reversible motor in either case would be directly proportional to the head or fall measured on a suitable scale. Caloric itself was not motive power, but must be regarded simply as the vehicle or carrier of energy, the production of motive power from caloric depending essentially (as Carnot puts it) not on the actual consumption of caloric, but on the fall of temperature available. The measure of a quantity of caloric is the work done per degree fall, which corresponds with the measure of a quantity of water by weight, *i. e.*, in kilogrammeters per meter fall.

That Carnot did not pursue the analogy further, and deduce the whole mechanical theory of heat from the caloric theory, is hardly to be wondered at if we remember that no applications of the energy principle had then been made in any department of physics. He appears, indeed, at a later date to have caught a glimpse of the general principle when he states that "motive power [his equivalent for work or energy] changes its form but is never annihilated." It is clear from the posthumous notes of his projected experimental work that he realized how much remained to be done on the experimental side, especially in relation to the generation of caloric by friction, and the waste of motive power by conduction of heat, which appeared to him (in 1824) "almost inexplicable in the present state of the theory of heat."

One of the points which troubled him most in the application of the theoretical result that the work obtainable from a quantity of caloric was simply proportional to the fall of temperature available, was that it required that the specific heat of a perfect gas should be independent of the pressure. This was inconsistent with the general opinion prevalent at the time, and with one solitary experiment by Delaroche and Bérard, which appeared to show that the specific heat of a gas diminished with increase of pressure, and which had been explained by Laplace as a natural consequence of the caloric theory. Carnot showed that this result did not necessarily follow from the caloric theory, but the point was not finally decided in his favor until the experiments of Regnault, first published in 1852, established the correct values of the specific heat of gases, and proved that they were practically independent of the pressure.

Another point which troubled Carnot was that, according to his calculations, the motive power obtainable from a kilocalorie of heat per degree fall appeared to diminish with rise of temperature, instead of remaining constant. This might have been due to experimental errors, since the data were most uncertain. But, if he had lived to carry out his projected experiments on the quantity of motive power required to produce one unit of heat, and had obtained the result, 424 kilogrammeters per kilocalorie, subsequently found by Joule, he could hardly have failed to notice that this was the same (within the limits of experimental error) as the maximum work AQT obtainable from the kilocalorie according to his equation. (This is seen to be the case when the values calculated by Carnot per degree fall at different temperatures were multiplied by the absolute temperature in each case. *E. g.*, 1.212 kilogrammeter per degree fall with steam at 79° C. or 352° Abs. $1.212 \times 352 = 426$ kilogram-

meters.) The origin of the apparent discrepancy between theory and experiment lay in the tacit assumption that the quantity of caloric in a kilocalorie was the same at different temperatures. There were no experiments at that time available to demonstrate that the caloric measure of heat as work per degree fall, implied in Carnot's principle, or more explicitly stated in his equation, was not the same as the calorimetric measure obtained by mixing substances at different temperatures. Even when the energy principle was established its exponents failed to perceive exactly where the discrepancy between the two theories lay. In reality both were correct, if fairly interpreted in accordance with experiment, but they depended on different methods of measuring a quantity of heat, which, so far from being inconsistent, were mutually complementary.

The same misconception, in a more subtle and insidious form, is still prevalent in such common phrases as the following: "We now know that heat is a form of energy and not a material fluid." The experimental fact underlying this statement is that our ordinary methods of measuring quantities of heat in reality measure quantities of thermal energy. When two substances at different temperatures are mixed, the quantity remaining constant, provided that due allowance is made for external work done and for external loss of heat, is the total quantity of energy. Heat is a form of energy merely because the thing we measure and call heat is really a quantity of energy. Apart from considerations of practical convenience, we might equally well have agreed to measure a quantity of heat in accordance with Carnot's principle, by the external work done in a cycle per degree fall. Heat would then not be a form of energy, but would possess all the properties postulated for caloric. The caloric measure of heat follows directly from Carnot's principle, just as the energy measure follows from the law of conservation of energy. But the term *heat* has become so closely associated with the energy measure that it is necessary to employ a different term, *caloric*, to denote the simple measure of a quantity of heat as opposed to a quantity of heat energy. The measurement of heat as caloric is precisely analogous to the measure of electricity as a quantity of electric fluid. In the case of electricity, the quantity measure is more familiar than the energy measure, because it is generally simpler to measure electricity by its chemical and magnetic effects as a quantity of fluid than as a quantity of energy. The units for which we pay by electric meter, however, are units of energy, because the energy supplied is the chief factor in determining the cost of production, although the actual quantity of fluid supplied has a good deal to do with the cost of distribution. Both methods of measurement are just as important in the theory of heat, and it seems a great pity that the natural measure of heat quantity is obscured in the elementary stages of exposition by regarding heat simply as so much energy. The inadequacy of such treatment makes itself severely felt in the later stages.

Since Carnot's principle was adopted without material modification into the mechanical theory of heat, it was inevitable that Carnot's caloric, and his solution for the work done in a finite cycle, should sooner or later be rediscovered. Caloric reappeared first as the "thermo-dynamic function" of Rankine, and as the "equivalence value of a transformation" in the equations of Clausius; but it was regarded rather as the quotient of heat energy by temperature than as possessing any special physical significance. At a later date,

when its importance was more fully recognized, Clausius gave it the name of *entropy*, and established the important property that its total quantity remained constant in reversible heat exchanges, but always increased in an irreversible process. Any process involving a decrease in the total quantity of entropy was impossible. Equivalent propositions with regard to the possibility or impossibility of transformations had previously been stated by Lord Kelvin in terms of the dissipation of available energy. But, since Carnot's solution had been overlooked, no one at the time seems to have realized that entropy was simply Carnot's caloric under another name, that heat could be measured otherwise than as energy, and that the increase of entropy in any irreversible process was the most appropriate measure of the quantity of heat generated. Energy so far as we know must always be associated with something of a material nature acting as carrier, and there is no reason to believe that heat energy is an exception to this rule. The tendency of the kinetic theory has always been to regard entropy as a purely abstract mathematical function, relating to the distribution of the energy, but having no physical existence. Thus it is not a quantity of anything in the kinetic theory of gases, but merely the logarithm of the probability of an arrangement. In a similar way, some twenty years ago the view was commonly held that electric phenomena were due merely to strains in the ether, and that the electric fluids had no existence except as a convenient means of mathematical expression. Recent discoveries have enabled us to form a more concrete conception of a charge of electricity, which has proved invaluable as a guide to research. Perhaps it is not too much to hope that it may be possible to attach a similar conception with advantage to caloric as the measure of a quantity of heat.

It has generally been admitted in recent years that some independent measure of heat quantity as opposed to heat energy is required, but opinions have differed widely with regard to the adoption of entropy as the quantity factor of heat. Many of these objections have been felt rather than explicitly stated, and are therefore the more difficult to answer satisfactorily. Others arise from the difficulty of attaching any concrete conception of a quantity of something to such a vague and shadowy mathematical function as entropy. The answer to the question "What is caloric?" must necessarily be of a somewhat speculative nature. But it is so necessary for the experimentalist to reason by analogy from the seen to the unseen, that almost any answer, however crude, is better than none at all. The difficulties experienced in regarding entropy as a measure of heat quantity are more of an academic nature, but may be usefully considered as a preliminary in attempting to answer the more fundamental question.

The first difficulty felt by the student in regarding caloric as the measure of heat quantity is that when two portions of the same substance, such as water, at different temperatures are mixed, the quantity of caloric in the mixture is greater than the sum of the quantities in the separate portions. The same difficulty was encountered by Carnot from the opposite point of view. The two portions at different temperatures represented a possible source of motive power. The question which he asked himself may be put as follows: "If the total quantity of caloric remained the same when the two portions at different temperatures were simply mixed, what had become of the motive power wasted?" The answer is that caloric is generated, and that the

quantity generated is such that its energy is the precise equivalent of the motive power which might have been obtained if the transfer of heat had been effected by means of a perfect engine working without generation of caloric. The caloric generated in wasting a difference of temperature is the necessary and appropriate measure of the quantity of heat obtained by the degradation of available motive power into the less available or transformable variety of heat energy.

The processes by which caloric is generated in mixing substances at different temperatures, or in other cases where available motive power is allowed to run to waste, are generally of so turbulent a character that the steps of the process can not be followed, although the final result can be predicted under given conditions from the energy principle. Such processes could not be expected *a priori* to throw much light on the nature of caloric. The familiar process of conduction of heat through a body, the parts of which are at different temperatures, while equally leading to the generation of a quantity of caloric equivalent to the motive power wasted, affords better promise of elucidating the nature of caloric, owing to the comparative simplicity and regularity of the phenomena, which permit closer experimental study. The earliest measurements of the relative conducting powers of the metals for heat and electricity showed that the ratio of the thermal to the electric conductivity was nearly the same for all the pure metals, and suggested that, in this case, the carriers of heat and electricity were the same. Later and more accurate experiments showed that the ratio of the conductivities was not constant, but varied nearly as the absolute temperature. At first sight this might appear to suggest a radical difference between the two conductivities, but it results merely from the fact that heat is measured as energy in the definition of thermal conductivity, whereas electricity is measured as a quantity of fluid. If thermal conductivity were defined in terms of caloric or thermal fluid, the ratio of the two conductivities would be constant with respect to temperature almost, if not quite, within the limits of error of experiment. On the hypothesis that the carriers are the same for electricity and heat, and that the kinetic energy of each carrier is the same as that of a gas molecule at the same temperature, it becomes possible, on the analogy of the kinetic theory of gases, to calculate the actual value of the ratio of the conductivities. The value thus found agrees closely in magnitude with that given by experiment, and may be regarded as confirming the view that the carriers are the same, although the hypotheses and analogies invoked are somewhat speculative.

When the electrons or corpuscles of negative electricity were discovered it was a natural step to identify them with the carriers of energy, and to imagine that a metal contained a large number of such corpuscles, moving in all directions, and colliding with each other, and with the metallic atoms, like the molecules of a gas on the kinetic theory. If the mass of each carrier were $\frac{1}{1700}$ of that of an atom of hydrogen, the velocity at 0° C. would be about sixty miles a second, and would be of the right order of magnitude to account for the observed values of the conductivities of good conductors, on the assumption that the number of negative corpuscles was the same as the number of positive metallic atoms, and that the mean free path of each corpuscle was of the same order as the distance between the atoms. The same hypothesis served to give a qualitative account of thermo-electric phenomena, such

as the Peltier and Thomson effects, and of radiation and absorption of heat, though in a less satisfactory manner. When extended to give a consistent account of *all* the related phenomena, it would appear that the number of free corpuscles required is too large to be reconciled, for instance, with the observed values of the specific heat, on the assumption that each corpuscle possesses energy of translation equal to that of a gas molecule at the same temperature.

Sir J. J. Thomson has accordingly proposed and discussed another possible theory of metallic conduction, in which the neutral electric doublets present in the metal are supposed to be continually interchanging corpuscles at a very high rate. Under ordinary condition these interchanges take place indifferently in all directions, but under the action of an electric field the axes of the doublets are supposed to become more or less oriented, as in the Grotthus-chain hypothesis of electrolytic conduction, producing a general drift or current proportional to the field. This hypothesis, though fundamentally different from the preceding or more generally accepted view, appears to lead to practically the same relations, and is in some ways preferable, as suggesting possible explanations of difficulties encountered by the first theory in postulating so large a number of free negative corpuscles. On the other hand, the second theory requires that each neutral doublet should be continually ejecting corpuscles at the rate of about 10^{15} per second. There are probably elements of truth in both theories, but, without insisting too much on the exact details of the process, we may at least assert with some confidence that the corpuscles of caloric which constitute a current of heat in a metal are very closely related to the corpuscles of electricity, and have an equal right to be regarded as constituting a material fluid possessing an objective physical existence.

If I may be allowed to speculate a little on my own account (as we are all here together in holiday mood, and you will not take anything I may say too seriously), I should prefer to regard the molecules of caloric, not as being identical with the corpuscles of negative electricity, but as being neutral doublets formed by the union of a positive and negative corpuscle, in much the same way as a molecule of hydrogen is formed by the union of two atoms. Nothing smaller than a hydrogen atom has yet, so far as I know, been discovered with a positive charge. This may be merely a consequence of the limitations of our experimental methods, which compel us to employ metals to so large an extent as electrodes. In the symmetry of nature it is almost inconceivable that the positive corpuscles should not exist, if only as the other end of the Faraday-tube or vortex-filament representing a chemical bond. Professor Bragg has identified the X or γ rays with neutral corpuscles traveling at a high velocity, and has maintained this hypothesis with brilliant success against the older view that these rays are not separate entities, but merely thin, spreading pulses in the ether produced by the collisions of corpuscles with matter. I must leave him to summarize the evidence, but if neutral corpuscles exist, or can be generated in any way, it should certainly be much easier to detach a neutral corpuscle from a material atom or molecule than to detach a corpuscle with a negative charge from the positive atom with which it is associated. We should therefore expect neutral corpuscles to be of such exceedingly common and universal occurrence

that their very existence might be overlooked, unless they happened to be traveling at such exceptionally high velocities as are associated with the γ rays. According to the pulse theory, it is assumed that all γ rays travel with the velocity of light, and that the enormous variations observed in their penetrative power depend simply on the thickness of the pulse transmitted. On the corpuscular theory, the penetrative power, like that of the α and β rays, is a question of size, velocity, and electric charge. Particles carrying electric charges, like the α and β rays, lose energy in producing ions by their electric field, perhaps without actual collision. Neutral or γ rays do not produce ions directly, but dislodge either γ rays or β rays from atoms by direct collisions, which are comparatively rare. The β rays alone, as C. T. R. Wilson's photographs show, are responsible for the ionization. Personally, I have long been a convert to Professor Bragg's views on the nature of X rays, but even if we regard the existence of neutral corpuscles as not yet definitely proved, it is, I think, permissible to assume their existence for purposes of argument, in order to see whether the conception may not be useful in the interpretation of physical phenomena.

If, for instance, we assume that these neutral corpuscles or molecules of caloric exist in conductors and metallic bodies in a comparatively free state of solution, and are readily dissociated into positive and negative electrons owing to the high specific inductive capacity of the medium, the whole theory of metallic conduction follows directly on the analogy of conduction in electrolytic solutions. But, whereas in electrolytes the ions are material atoms moving through a viscous medium with comparatively low velocities, the ions in metallic conductors are electric corpuscles moving with high velocities more after the manner postulated in the kinetic theory of gases. It is easy to see that this theory will give similar numerical results to the electronic theory when similar assumptions are made in the course of the work. But it has the advantage of greater latitude in explaining the vagaries of sign of the Hall effect, and many other peculiarities in the variation of resistance and thermo-electric power with temperature. For good conductors, like the pure metals, we may suppose, on the electrolytic analogy, that the dissociation is practically complete, so that the ratio of the conductivities will approach the value calculated on the assumption that all the carriers of heat are also carriers of electricity. But in bad conductors the dissociation will be far from complete, and it is possible to see why, for instance, the electric resistance of cast iron should be nearly ten times that of pure iron, although there is comparatively little difference in their thermal conductivities. The numerical magnitude of the thermo-electric effect, which is commonly quoted in explanation of the deviation of alloys from the electronic theory, is far too small to produce the required result; and there is little or no correspondence between the thermo-electric properties of the constituents of alloys and the variations of their electric conductivities.

One of the oldest difficulties of the material theory of heat is to explain the process of the production of heat by friction. The application of the general principle of the conservation of energy leads to the undoubted conclusion that the thermal energy generated is the equivalent of the mechanical work spent in friction, but throws little or no light on the steps of the process, and gives no information with regard to the actual nature of the energy

produced in the form of heat. It follows from the energy principle that the quantity of caloric generated in the process is such that its total energy at the final temperature is equal to the work spent. If a quantity of caloric represents so many neutral molecules of electricity, one can not help asking where they came from, and how they were produced. It is certain that in most cases of friction, wherever slip occurs, some molecules are torn apart, and the work spent is represented in the first instance by the separation of electric ions. Some of these ions are permanently separated as frictional electricity, and can be made to perform useful work; but the majority recombine before they can be effectively separated, leaving only their equivalent in thermal energy. The recombination of two ions is generally regarded simply as reconstituting the original molecule at a high temperature, but in the light of recent discoveries we may perhaps go a step further. It is generally admitted that X or γ rays are produced by the sudden stoppage of a charged corpuscle, and Lorentz, in his electron theory of radiation, has assumed that such is the case however low the velocity of the electron. A similar effect must occur in the sudden stoppage of a pair of ions rushing together under the influence of their mutual attraction. Rays produced in this way would be of an exceedingly soft or absorbable character, but they would not differ in kind from those produced by electrons except that their energy, not exceeding that of a pair of ions, would be too small to produce ionization, so that they could not be detected in the usual way. If the X rays are corpuscular in their nature, we can not logically deny the corpuscular character even to the slowest moving rays. We know that X rays continually produce other X rays of lower velocity. The final stage is probably reached when the average energy of an X corpuscle or molecule of caloric is the same as that of a gas molecule at the same temperature, and the number of molecules of caloric generated is such that their total energy is equal to the work originally spent in friction.

In this connection it is interesting to note that Sir J. J. Thomson, in a recent paper on "Ionization by Moving Particles," has arrived, on other grounds, at the conclusion that the character of the radiation emitted during the recombination of the ions will be a series of pulses, each pulse containing the same amount of energy and being of the same type as very soft X rays. If the X rays are really corpuscular, these definite units or quanta of energy generated by the recombination of the ions bear a close resemblance to the hypothetical molecules of caloric.

It may be objected that in many cases of friction, such as internal or viscous friction in a fluid, no electrification or ionization is observable, and that the generation of caloric can not in this case be attributed to the recombination of ions. It must, however, be remarked that the generation of a molecule of caloric requires less energy than the separation of two ions; that, just as the separation of two ions corresponds with the breaking of a chemical bond, so the generation of one or more molecules of caloric may correspond with the rupture of a physical bond, such as the separation of a molecule of vapor from a liquid or solid. The assumption of a molecular constitution for caloric follows almost of necessity from the molecular theories of matter and electricity, and is not inconsistent with any well-established experimental facts. On the contrary, the many relations which are known to exist between the specific heats of similar substances, and also between the latent

heats, would appear to lead naturally to a molecular theory of caloric. For instance, it has often been noticed that the molecular latent heats of vaporization of similar compounds at their boiling points are proportional to the absolute temperature. It follows that the molecular latent caloric of vaporization is the *same* for all such compounds, or that they require the same number of molecules of caloric to effect the same change of state, irrespective of the absolute temperatures of their boiling points. From this point of view one may naturally regard the liquid and gaseous states as conjugate solutions of caloric in matter and matter in caloric respectively. The proportion of caloric to matter varies regularly with pressure and temperature, and there is a definite saturation limit of solubility at each temperature.

One of the most difficult cases of the generation of caloric to follow in detail is that which occurs whenever there is exchange of heat by radiation between bodies at different temperatures. If radiation is an electro-magnetic wave-motion, we must suppose that there is some kind of electric oscillator or resonator in the constitution of a material molecule which is capable of responding to the electric oscillations. If the natural periods of the resonators correspond sufficiently closely with those of the incident radiation the amplitude of the vibration excited may be sufficient to cause the ejection of a corpuscle of caloric. It is generally admitted that the ejection of an electron may be brought about in this manner, but it would evidently require far less energy to produce the emission of a neutral corpuscle, which ought therefore to be a much more common effect. On this view, the conversion of energy of radiation into energy of caloric is a discontinuous process taking place by definite molecular increments, but the absorption or emission of radiation itself is a continuous process. Professor Planck, by a most ingenious argument based on the probability of the distribution of energy among a large number of similar electric oscillators (in which the entropy is taken as the logarithm of the probability, and the temperature as the rate of increase of energy per unit of entropy), has succeeded in deducing his well-known formula for the distribution of energy in full radiation at any temperature; and has recently, by a further extension of the same line of argument, arrived at the remarkable conclusion that, while the absorption of radiation is continuous, the emission of radiation is discontinuous, occurring in discrete elements or quanta. Where an argument depends on so many intricate hypotheses and analogies the possible interpretations of the mathematical formulæ are to some extent uncertain; but it would appear that Professor Planck's equations are not necessarily inconsistent with the view above expressed that both emission and absorption of radiation are continuous, and that his *elementa quanta*, the energy of which varies with their frequency, should rather be identified with the molecules of caloric, representing the conversion of the electro-magnetic energy of radiation into the form of heat, and possessing energy in proportion to their temperature.

Among the difficulties felt rather than explicitly stated, in regarding entropy or caloric as the measure of heat quantity, is its awkward habit of becoming infinite, according to the usual approximate formulæ, at extremes of pressure or temperature. If caloric is to be regarded as the measure of heat quantity, the quantity existing in a finite body must be finite, and must vanish at the absolute zero of temperature. In

reality there is no experimental foundation for any other conclusion. According to the usual gas formulæ it would be possible to extract an infinite quantity of caloric from a finite quantity of gas by compressing it at constant temperature. It is true that (even if we assumed the law of gases to hold up to infinite pressures, which is far from being the case) the quantity of caloric extracted would be of an infinitely low order of infinity as compared with the pressure required. But, as a matter of fact, experiment indicates that the quantity obtainable would be finite, although its exact value can not be calculated owing to our ignorance of the properties of gases at infinite pressures. In a similar way, if we assume that the specific heat as ordinarily measured remains constant, or approaches a finite limit at the absolute zero of temperature, we should arrive at the conclusion that an infinite quantity of caloric would be required to raise the temperature of a finite body from 0° to 1° absolute. The tendency of recent experimental work on specific heats at low temperatures, by Tilden, Nernst, Lindemann and others, is to show, on the contrary, that the specific heats of all substances tend to vanish as the absolute zero is approached and that it is the specific capacity for caloric which approaches a finite limit. The theory of the variation of the specific heats of solids at low temperatures is one of the most vital problems in the theory of heat at the present time, and is engaging the attention of many active workers. Professor Lindemann, one of the leading exponents of this work, has kindly consented to open a discussion on the subject in our section. We are very fortunate to have succeeded in securing so able an exponent, and shall await his exposition with the greatest interest. For the present I need only add that the obvious conclusion of the caloric theory bids fair to be completely justified.

A most interesting question, which early presented itself to Rumford and other inquirers into the caloric theory of heat, was whether caloric possessed *weight*. While a positive answer to this question would be greatly in favor of a material theory, a negative answer, such as that found by Rumford, or quite recently by Professor Poynting and Phillips, and by Mr. L. Southerns working independently, would not be conclusively against it. The latter observers found that the change in weight, if any, certainly did not exceed 1 in 10^8 per 1° C. If the mass of a molecule of caloric were the same as that generally attributed to an electron, the change of weight, in the cases tested, should have been of the order of 1 in 10^7 per 1° C., and should not have escaped detection. It is generally agreed, however, that the mass of the electron is entirely electro-magnetic. Any such statement virtually assumes a particular distribution of the electricity in a spherical electron of given size. But if electricity itself really consists of electrons, an argument of this type would appear to be so perfectly circular that it is questionable how much weight should be attached to it. If the equivalent mass of an electron in motion arises solely from the electromagnetic field produced by its motion, a neutral corpuscle of caloric should not possess mass or energy of translation as a whole, though it might still possess energy of vibration or rotation of its separate charges. For the purpose of mental imagery we might picture the electron as the free or broken end of a vortex filament, and the neutral corpuscle as a vortex ring produced when the positive and negative ends are united; but a mental picture of this kind does not carry us any further than the

sphere coated with electricity, except in so far as either image may suggest points for experimental investigation. In our ignorance of the exact mechanism of gravity it is even conceivable that a particle of caloric might possess mass without possessing weight, though, with the possible exception of the electron, nothing of the kind has yet been demonstrated. In any case it would appear that the mass, if any, associated with a quantity of caloric must be so small that we could not hope to learn much about it by the direct use of the balance.

The fundamental property of caloric, that its total quantity can not be diminished by any known process and that it is not energy but merely the vehicle or carrier of energy, is most simply represented in thought by imagining it to consist of some indestructible form of matter. The further property, that it is always generated in any turbulent or irreversible process, appears at first sight to conflict with this idea, because it is difficult to see how anything indestructible can be so easily generated. When, however, we speak of caloric as being generated, what we really mean is that it becomes associated with a material body in such a way that we can observe and measure its quantity by the change of state produced. The caloric may have existed previously in a form in which its presence could not be detected. In the light of recent discoveries we might suppose the caloric generated to arise from the disintegration of the atoms of matter. No doubt some caloric is produced in this way, but those corpuscles that are so strongly held as to be incapable of detection by ordinary physical methods require intense shocks to dislodge them. A more probable source of caloric is the ether, which, so far as we know, may consist entirely of neutral corpuscles of caloric. The hypothesis of a continuous ether has led to great difficulties in the electro-magnetic theory of light and in the kinetic theory of gases. A molecular, or cellular-vortex, structure appears to be required. According to the researches of Kelvin, Fitzgerald and Hicks, such an ether can be devised to satisfy the requirements of the electro-magnetic theory without requiring it to possess a density many times greater than that of platinum. So far as the properties of caloric are concerned, a neutral pair of electrons would appear to constitute the simplest type of molecule, though without more exact knowledge of the ultimate nature of an electric charge it would be impossible to predict all its properties. Whether an ether composed of such molecules would be competent to discharge satisfactorily all the onerous functions expected from it, may be difficult to decide, but the inquiry, in its turn, would probably throw light on the ultimate structure of the molecule.

Without venturing too far into the regions of metaphysical speculation, or reasoning in vicious circles about the nature of an electric charge, we may at least assert with some degree of plausibility that material bodies under ordinary conditions probably contain a number of discrete physical entities, similar in kind to X rays or neutral corpuscles, which are capable of acting as carriers of energy, and of preserving the statistical equilibrium between matter and radiation at any temperature in virtue of their interchanges with electrons. If we go a step further and identify these corpuscles with the molecules of caloric, we shall certainly come in conflict with some of the fundamental dogmas of the kinetic theory, which tries to express everything in terms of energy, but the change involved is mainly one of standpoint or expression. The experimental facts remain the same,

but we describe them differently. Caloric has a physical existence, instead of being merely the logarithm of the probability of a complexion. In common with many experimentalists, I can not help feeling that we have everything to gain by attaching a material conception to a quantity of caloric as the natural measure of a quantity of heat as opposed to a quantity of heat energy. In the time at my disposal I could not pretend to offer you more than a suggestion of a sketch, an apology for the possibility of an explanation, but I hope I may have succeeded in conveying the impression that a caloric theory of heat is not so entirely unreasonable in the light of recent experiment as we are sometimes led to imagine.

H. L. Callendar

THE PROBLEM OF MECHANICAL FLIGHT

HISTORICAL RÉSUMÉ

The scientific period in aviation began in 1809 when Sir George Cayley published in *Nicholson's Journal* the first complete mechanical theory of the aeroplane, in which he put clearly in evidence the fundamental principle of sustention obtained by velocity. This memoir passed unnoticed until unearthed some sixty years later by Pénaud. Following Cayley there was a long unfruitful interval in which fell the projected aeroplane of Henson in 1842–43, the attempts at gliding by Le Bris in 1856, and the biplane gliders of Wenham in 1866. At the end of the Franco-Prussian war interest in heavier-than-air flying machines was revived, and the Société française de Navigation aérienne was from 1872 on composed of a number of investigators engaged in the conquest of the air. The history of their endeavors is found in "L'Aéronaute." Among them was Alphonse Pénaud, a young mechanic whose early death prevented him from pushing his researches to their logical end. Pénaud was less isolated than Cayley and one of his memoirs was crowned by the Académie des Sciences. He constructed the first toy aeroplane, with the propeller in the rear and driven by a rubber band. This apparatus flew for an appreciable time, *utilizing motive energy which it carried with it,* and this property differentiates very sharply the experiment of Penaud from those of his predecessors, in which was realized only a fall more or less retarded by the air.

The German Lilienthal followed Penaud, and from 1891 studied the equilibrium, maneuvering and landing of gliders, falling to his death on his two thousandth flight, August 9, 1896. In this country the French engineer Chanute and the American Langley had meanwhile been experimenting and developing the laws of aerodynamics, Langley's work going as far back as 1887 and continuing until his unsuccessful attempts at flight in 1903. In 1891 he published[1] the results of his researches, and definitely stated that it was possible to construct machines which would give such velocity to inclined surfaces that bodies indefinitely heavier than air could be sustained upon it and moved through it with great speed.

By the end of the nineteenth century efforts to build aeroplanes had become numerous. Sir Hiram Maxim in England and Ader in France both constructed machines and made attempts to fly them. Maxim built in 1890–95 a flying machine with 557 square meters of surface and 3,640 kilograms weight, which was damaged before leaving the ground and abandoned. The "Avion" of Ader was tested on the field of Satory in 1897 before the representatives of the French War Department, but its performance led the department to withdraw its support and experiments were discontinued. Langley as early as 1896 had designed and built a small steam-driven model aerodrome weighing about 13 kilograms, and on May 6 of that year he flew it some 1,200 meters over the waters of the Potomac. The quarter-size model of his large man-carrying aerodrome flew successfully about 1,000 feet near Widewater, Va., on August 8, 1903, but the large machine itself, carrying Mr. Manly, was injured in launching

[1] "Experiments in Aerodynamics," Smithsonian Contributions to Knowledge, Vol. 27, 1891.

from the top of a houseboat on October 17, 1903, and wrecked in a second attempt on December 8 of that same year.

With this ended sixteen years of effort on the part of Langley to attain mechanical flight, and his long period of fruitful scientific achievement closed with failure due primarily to lack of funds. The report[2] of Major Macomb to the War Department concerning the tests made on the Potomac is interesting and illuminating.

After describing the attempted launching on October 7 and the subsequent wrecking of the great aerodrome on December 8, Major Macomb closes with the following paragraphs:

Having reached the present stage of advancement in its development, it would seem highly desirable, before laying down the investigation, to obtain conclusive proof of the possibility of free flight, not only because there are excellent reasons to hope for success, but because it marks the end of a definite step toward the attainment of the final goal.

In the meantime, to avoid any possible misunderstanding, it should be stated that even after a successful test of the present great aerodrome, designed to carry a man, we are still far from the ultimate goal, and it would seem as if years of constant work and study by experts, together with the expenditure of thousands of dollars, would still be necessary before we can hope to produce an apparatus of practical utility on these lines.

The War Department had made two allotments of $25,000 each to Langley to further his experiments, one in 1898 and the second in 1899. The large aerodrome was completed in July of 1903, the long delay being due to Langley's inability to secure a suitable motor, which he finally had to design. The tests referred to above were made in the fall of 1903 and on March 3, 1904, the Board of Ordnance and Fortifications stated[3] that it was "not prepared to make an additional allotment at this time for continuing the work." All material was left in Langley's possession for such future experiments as he might wish to make.

[2] Langley, "Memoir on Mechanical Flight," p. 276, Smithsonian Contributions to Knowledge, Vol. 27, 1911.

[3] Langley, "Memoir," p. 278.

On November 14, 1908, the board again reported:[4]

Doctor Langley considered it desirable to continue the experiments, but the Board deemed it advisable, largely in view of the adverse opinions expressed in Congress and elsewhere, to suspend operations in that direction.

The persistent misrepresentations of the public press caused Langley to publish the following statement.

SMITHSONIAN INSTITUTION,
WASHINGTON, D. C.,
August 19, 1903.

TO THE PRESS: The present experiments being made in mechanical flight have been carried on partly with funds provided by the Board of Ordnance and Fortification and partly from private sources, and from a special endowment of the Smithsonian Institution. The experiments are carried on with the approval of the Board of Regents of the Smithsonian Institution.

The public's interest in them may lead to an unfounded expectation as to their immediate results, without the explanation which is here briefly given.

These trials, with some already conducted with steam-driven flying machines, are believed to be the first in the history of invention where bodies, far heavier than air itself, have been sustained in the air for more than a few seconds by purely mechanical means.

On my previous trials, success has only been reached after initial failures, which alone have taught the way to it, and I know no reason why the prospective trials should be an exception.

It is possible, rather than probable, that it may be otherwise now, but judging them from the light of past experience, it is to be regretted that the enforced publicity which has been given to these initial experiments, which are essentially experiments and nothing else, may lead to quite unfounded expectations.

It is the practice of all scientific men, indeed of all prudent men, not to make public the results of their work till these are certain. This consideration, and not any desire to withhold from the public matters in which the public is interested, has dictated the policy thus far pursued here. The fullest publicity, consistent with the national interest (since these recent experiments have for their object the development of a machine for war

[4] Langley, "Memoir," p. 279.

purposes), will be given to this work when it reaches a stage which warrants publication.

(Signed) S. P. Langley

Like many men of his kind Langley seems to have had that passionate sense of privacy which resents alike the curiosity of the public and the sensationalism of the newspapers. The antagonistic attitude of the press gave a character of finality to experiments which to Langley himself were but members of a long series bringing him each year nearer the goal. Had his health and strength remained to combat the hostility of press and public, he would in all probability have gone on to success, undeterred by criticism and misunderstanding. To such patient and unremitting labor as his is owed the accomplished fact of mechanical flight. He began his investigations at a time when even progressive men of science thought flying a wild dream, and a large part of that exact investigation which transformed vague ideas into scientific knowledge is due directly to him. His work is measured not solely by his contributions, but much more by the unswerving advance in a field of scientific inquiry in which no road was marked.

The spirit of the man is made evident by his steadfast refusal to entertain propositions made him to assist in the development of the aerodrome provided arrangements were made for later commercialization. He had given his time and energy without hope of remuneration, and even when no assistance could be obtained from any other source and success seemed but a step away, he could not bring himself to capitalize his scientific work, although his age was such that any delay in achieving success increased the probability of his not living to see it. He died February 27, 1906, about two years before the Wright brothers astonished the world by their feats in sustained flying in 1908.

II

FUTURE OF THE AEROPLANE

The Problem of Velocity.—The mechanical theory of the behavior of the aeroplane is built on the principle that in steady horizontal flight the normal thrust on the sustaining surface is proportional to the area, to the square of the relative velocity, and to the sine of the angle of attack, or:

$$(1) \qquad F = kSV^2 \sin i,$$

where, *for small values of* i, k is a constant for a given surface under constant atmospheric conditions, S is the area of the sustaining surface, V its velocity relative to the air stream, and i the angle of attack. For uniform horizontal flight i equals the fixed inclination of the sustaining surface to the horizontal axis of the aeroplane, and is a constant of the machine.

The vertical component of this normal thrust—*the lift*—must equal the weight of the whole machine, and denoting the lift by L and the weight by W, there follows:

$$(2) \quad L = W = kSV^2 \sin i \cos i = kSV^2 i \text{ (nearly)}.$$

The *sustaining resistance* R is given by the horizontal component of the normal thrust, whence:

$$(3) \qquad R = kSV^2 \sin^2 i = kSV^2 i^2 \text{ (nearly)}.$$

The power P required to overcome the sustaining resistance R is:

$$(4) \qquad P = RV = kSV^3 i^2.$$

From (2) and (4) we have:

$$(5) \qquad \begin{aligned} V^2 &= W/kSi, \\ P &= W^2/ksV. \end{aligned}$$

These two relations lead to the very important result that the *velocity of sustention* V increases as the angle of attack decreases, and the power required to drive the sustaining plane against its own resistance decreases as the velocity increases. The advantage of flying at high velocity and "close to the wind" is at once evident.

In these relations, however, the resistance offered by the motor and its accessory parts including the framework—*the passive resistance*—has been neglected. The power required to overcome this is proportional to the cube of the velocity and the "equivalent surface" presented to the air stream, and is enormously increased at high velocities. It is at once evident that for a given machine there is a maximum velocity beyond which the motor can

not drive it. The *problem of velocity* may then be stated as follows: Required a motor which shall be capable of driving itself and accessory parts, including framework and sustaining surface, against its own resistance at high velocity. Undoubtedly the problem so stated is too simplified, but the motor must be capable of developing at least that power.

The solution of the problem does not lie in the construction of larger motors homothetic to those now in use, for the "equivalent resisting surface" is increased at the same time that the power is augmented, and if the calculated attainable speed be based on the assumptions of power proportional to weight, weight proportional to cube of linear dimensions, and "equivalent resisting surface" proportional to square of linear dimensions, then the power must be increased 512 fold in order to double the speed. In particular, if a motor developing 100 horse power could drive an aeroplane at a speed of 100 kilometers an hour, a homothetic motor developing 800 horse power would, under the conditions stated above, drive its aeroplane at 126 kilometers per hour.

These assumptions are at the present time well within the range of consideration, and give a general idea of what to expect along the line of motor development.

The solution of the problem may, however, be in quite another direction: in the construction of steam line body forms for the aeroplanes of the future. Stream line forms offer a minimum resistance at high velocities, and their attainment is the immediate problem of the future so far as the development of high speeds is concerned. The architecture of the aeroplane is thus seen to be of paramount importance, and it is in that direction that future advance may be looked for.

The Problem of Stability.—More important than the attainment of high velocity is the realization of stability in flight. At the present time it is to a very large extent dependent on the personal skill of the aviator, and however great this may become, it is highly desirable that the aeroplane should be rendered *automatically stable* in straightaway flight at least, if for no other reason than to leave the aviator free to attend to such other matters as may legitimately engage his attention.

The distribution of the mass of the aeroplane about its center of gravity is at once felt in the sensitiveness of the response which the machine accords to disturbing forces. If the aeroplane be disturbed by some external force so that the angle of attack becomes a instead of i, it will oscillate about its position of equilibrium under the equation:

$$(6) \qquad I\frac{d^2a}{dt^2} = \sigma V^2(a - i) - 2hV\frac{da}{dt},$$

where I is the moment of inertia about a horizontal gravity axis perpendicular to the direction of motion, and σ and h are constants. This equation shows at once that if the sustaining plane pitches slightly, the *initial* oscillation will be the more violent the smaller the value of I and hence the closer the heavy masses to the center of gravity G. A more complete discussion of equation (6), however, shows that the motion defined by it, under the initial conditions

$$a_0 \neq i, \quad \left(\frac{da}{dt}\right)_0 = 0, \quad t = 0,$$

dies down the more rapidly the smaller the value of I, and hence the initial disadvantage of violent oscillation is more than compensated by the rapidity with which these oscillations disappear under *damping*. From the standpoint of stability the best type of machine would seem to be that in which the heavy masses are concentrated in the neighborhood of the center of gravity. Such a distribution, however, produces a machine very sensitive to external disturbances and too small a value of I will produce too great an initial value of a, and equation (6) will no longer define the motion. Theoretically, at least, under equation (6) the moment of inertia of a given machine might be made so small that the damping would produce a periodic motion, but practically the initial displacement would then be so large for a small force that the orientation would no longer be in the neighborhood of the equilibrium orientation. It is necessary that some

method of keeping the initial displacement small under a disturbing force be devised, but it is equally undesirable that the moment of inertia be materially increased by the introduction of the stabilizing device. This consideration alone would serve to discard all methods of stabilization making use of heavy masses, such as heavy gyroscopes or pendulums, and an effective stabilizing device would have to call into play the stabilizing surface by means of a mechanism of transmission operated by a light mass sensitive to light disturbing forces, such as a small but rapidly rotating gyroscope. Direct stabilization by a heavy pendular mass, for instance, is a purely chimerical procedure.

G. O. James
Washington University

EARLY MAN IN SOUTH AMERICA

Five years ago the Bureau of American Ethnology published a bulletin on Skeletal Remains Suggesting, or Attributed to, Early Man in North America, based on the researches of Dr. Ales Hrdlička, Curator of Physical Anthropology in the U. S. National Museum. There is to appear shortly in similar form, under the title of Early Man in South America, a résumé of the investigations of Dr. Hrdlička, in collaboration with Mr. W. H. Holmes, head curator of the Department of Anthropology in the U. S. National Museum, Mr. Bailey Willis, of the U. S. Geological Survey, and Messrs. Fred. Eugene Wright and Clarence E. Fenner, of the Geophysical Laboratory of the Carnegie Institution of Washington.

Even before the completion of his report on ancient man in North America, Dr. Hrdlička became interested in the evidence bearing on the corresponding problem in South America, and subsequently, at the suggestion of Mr. W. H. Holmes, he was sent by the secretary of the Smithsonian Institution to visit Argentina for the purpose of making a study at first hand of the available material and an investigation of the most promising regions.

In view of the important position occupied by geology in studies of this nature, Mr. Bailey Willis of the U. S. Geological Survey was chosen to accompany Dr. Hrdlička.

The chief objects of the expedition were: the examination of the skeletal remains relating to early man, in Brazil and Argentina; the study of the principal localities and deposits from which these finds came; and, if possible, the collection of osseous, archeologic and other specimens bearing on the subject of man's antiquity. It was hoped that thorough investigation on the ground would enable the explorers to form more definite conclusions concerning the finds than the literature relating to them warranted, and that possibly by means of new discoveries additional light would be thrown on the whole subject of early man in South America, especially in Argentina.

The party reached Argentina early in May, 1910. Dr. Hrdlička spent two months in that country, while Mr. Willis remained somewhat longer, nearly all of this time being given to the researches recorded in the report. The work was greatly facilitated by several of the local men of science, and the authors express warm appreciation for the valuable assistance thus rendered. During the first part of the stay in Argentina, Dr. Hrdlička devoted his time to the study of the available skeletal material attributed to ancient man, found in the various local museums, while Mr. Willis examined the various samples of baked earth, and other objects believed to have been associated with the activities of prehistoric man. Several localities in Buenos Aires where local exposures could be studied, including the drydock where the "Diprothomo" skull had been found some time before, were carefully examined. On May 24 the party set out for the coast where important specimens had been discovered, and a few days later were joined at Mar del Plata, by the late Professor Florentino Ameghino and his brother Carlos, who assisted the expedition materially, accompanying Dr. Hrdlička and Mr. Willis for more than three weeks from place to place on the coast, and to several inland points of interest.

After the completion of this general survey, Dr. Hrdlička visited the valley of the Rio Negro whence came several fossil crania many years ago, while Mr. Willis proceeded to

Arroyo Siasgo and Alvear, to study the geology of these territories and several specimens of baked earth supposed to be the product of ancient human industry. Early in July, both explorers met again in Buenos Aires, and after finishing their work in that region started for Ovejero, a locality in northwestern Argentina that has come into prominence in the last few years through its yield of human bones; they also visited Tacuman, San Juan and Mendoza. Dr. Hrdlička then proceeded to Peru while Mr. Willis returned to Buenos Aires.

The researches occupied nearly three months. Every specimen relating to ancient man that could be found was examined and every important locality was investigated. Unfortunately the general results of the inquiry are not in harmony with claims previously made by the various authors who reported the several finds. On the contrary, the conclusion was reached that to the present time the evidence is unfavorable to the hypothesis of man's great antiquity, especially as to the existence of very early predecessors of the Indian in South America; nor does it sustain the theories of the evolution of man in general, or even that of an American race alone, in the southern continent. The facts gathered attest everywhere merely the presence of the already differentiated and relatively modern American Indian. This should not be taken as a categorical denial of the existence of early man in South America, however improbable such a conclusion may now appear; but the position is maintained that the final acceptance of the evidence on this subject can not be justified until there is accumulated a mass of strictly scientific observations, requisite in kind and volume, to establish a proposition of so great importance.

The expedition secured numerous geological, paleontological and anthropological specimens, some of which throw light on the question under investigation. All these specimens have been deposited in the United States National Museum for further study and exhibition.

THE NEW ALLEGHENY OBSERVATORY

THE new Allegheny Observatory, situated in Riverview Park, Pittsburgh, was dedicated on the afternoon of Wednesday, August 28, in the presence of the members of the Astronomical and Astrophysical Society of America, and of many of the Pittsburgh friends of the institution. The principal instruments of the new observatory are a 13-inch visual refractor, a 30-inch reflector (a memorial to James Edward Keeler), and a 30-inch photographic refractor (a memorial to William Thaw and his son, William Thaw, Junior). The last of these telescopes is not quite completed, as the objective remains to be supplied. Addresses were made by Dr. John A. Brashear, chairman of the observatory committee; by Dr. Samuel Black McCormick, chancellor of the University of Pittsburgh, of which the observatory forms the astronomical department; by Dr. Frank Schlesinger, director of the Allegheny Observatory, and by Professor E. C. Pickering, director of the Harvard College Observatory. Mrs. William Reed Thompson, the daughter of William Thaw and the sister of William Thaw Junior, closed the exercises with the unveiling of the memorial tablet on the Thaw telescope.

SCIENTIFIC NOTES AND NEWS

DR. W J MCGEE, known for his contributions to geology, anthropology and the conservation of natural resources, died at Washington on September 5, aged fifty-nine years.

DR. M. PLANCK, professor of theoretical physics in the University of Berlin, has been elected permanent secretary of the mathematical and physical section of the Berlin Academy of Sciences.

DR. JEAN MASCART, of the Paris Observatory, has been appointed director of the Lyons Observatory in succession to M. André.

IT was stated in last week's issue of SCIENCE that the friends and former students of Professor Wilhelm Wundt had presented to him on his eightieth birthday a foundation for the University of Leipzig. The disposition of the foundation was left to Professor Wundt, who

has decided to use it to equip in the psychological laboratory at Leipzig a department for psychological acoustics and phonetics.

Professor Émile Borel, of the University of Paris, and director of the *Revue du Mois* and of the *Nouvelle Collection Scientifique*, who is to be present at the opening of the Rice Institute of Houston, Texas, will be the guest of the University of Illinois part of the week beginning October 14.

Dr. R. Tait McKenzie, professor of physical education at the University of Pennsylvania, has executed a large bronze medallion known as "The Joy of Effort" which has been presented to the Swedish nation by the American committee on the Olympic games and is now being mounted in granite in the wall of the stadium at Stockholm.

Dr. Henry Fox, professor of biology at Ursinus College, has resigned to accept the position of assistant in entomology in the Bureau of Entomology. For the present he will be engaged at the permanent station of Lafayette, Indiana.

Dr. R. Kent Beattie, recently professor of botany in the State College of Washington, Pullman, Wash., has accepted a position as expert in the office of forest pathology of the Bureau of Plant Industry.

Mr. Harold Bryant, M.S. (Calif.), has been reappointed fellow in applied zoology in the University of California on the State Fish and Game Foundation for the continuance of an investigation into the economic status of the meadow lark, begun last year under the same appointment. Mr. Frank C. Clarke, M.S. (Calif.), has been appointed to a similar fellowship for the investigation of the problem of deer conservation in the state of California. The state commission and the department of zoology at the university cooperate in the investigation with a view to determining a scientific basis for legislation.

T. A. Bendrat, who has been engaged in teaching economic geology at the University of North Carolina, sailed from New York on the *Saramacca* on June 12 for Venezuela, where he will enter an engagement as reconnaisance geologist for at least two years.

While Professor E. J. McCaustland, Mem. Am. Soc. C. E., was prosecuting some water-power investigations on the Deschutes River in Oregon the boat in which he and two men were traveling was swamped in the rapids and his two companions were drowned. Professor McCaustland clung to the boat and was carried nearly a mile down the river and finally flung upon a rock in mid-stream. Three hours later he was rescued from this position by some men, who succeeded in getting a line out to him and who pulled him to the shore. The drowned men had both been selected to handle the boat on account of their long experience in river work. Both were excellent swimmers, but were unable to make shore on account of eddies and cross-currents.

Professor Wilhelm Ostwald, formerly of the University of Leipzig, will deliver a course of lectures at the University of Illinois during the week beginning October 13. The titles of the lectures will be announced later.

The following provisional program of lectures before the Harvey Society, New York, for the season of 1912–13 has been arranged:

October 5—Professor Max Rubner, University of Berlin: "Modern steam sterilization."

November 9—Professor Joseph Erlanger, George Washington University: "The localization of impulse initiation and conduction in the heart."

November 23—Professor G. N. Stewart, Western Reserve University: "The rate of the blood flow and the vasomotor reflexes in disease."

December 14: Professor F. B. Mallory, Harvard University: "The infectious lesions of blood vessels."

January 18—Major J. J. Russell, U.S.A.: "The prevention of typhoid fever."

February 15—Professor Theodore C. Janeway, Columbia University: "Nephritic hypertension: clinical and experimental studies."

March 1—Professor Edward G. Conklin, Princeton University: "The size of organisms and their constituent parts in relation to longevity, senescence and rejuvenescence."

March 22—Professor John Howland, Johns Hop-

kins University: "The scientific basis for the artificial feeding of infants."

THE Society of the Liebig Museum held its second meeting in the Liebig Laboratory at Giessen. It will be remembered that this society purchased last year the laboratory for fifteen thousand dollars. Many gifts connected with the life of the great chemist have been received.

REV. G. W. TAYLOR, curator of the Canadian Marine Biological Station at Departure Bay, near Nanaimo, B. C., died at his residence near the station on August 22. Mr. Taylor has been most widely known in the fields of entomology and conchology, but his interests, particularly as a collector, extended over the whole range of systematic zoology.

M. LUCIEN LÉVY, the distinguished French mathematician, has died at the age of fifty-nine years.

DR. FRANÇOIS ALPHONSE FOREL, known for his researches on the Lake of Geneva and on glaciers, died at Berne on August 8, aged seventy-one years.

MR. ALEXANDER DEAN, a British authority on horticulture, has died at the age of eighty years.

MR. A. BROTHERS, of Manchester, the author of several works and inventions concerned with photography, has died at the age of eighty-six years.

DR. ANDREW WILSON, lecturer on physiology and health under the George Combe trust and the author of numerous books and articles for the popularization of science, died on August 25.

WE are informed that a number of thefts of platinum from laboratories have occurred recently, and as a warning to those responsible we are requested to publish the following description of the thief: "A German Jew, with a German accent; age, about 40 years; height, 5 feet 8 inches; weight, about 200 pounds; peculiar shaped head; bald; clean shaven; large, short neck; heavy jaws; dark complected; dark hair and eyes; intelligent; when last seen wore blue serge suit and panama hat, and representing himself to be a book and magazine agent."

WE learn from *Nature* that the collection of foreign Lepidoptera bequeathed by the late Mr. H. T. Adams, of Enfield, has been received at the Natural History Branch of the British Museum. It is contained in 68 cabinets, and is stated to comprise about 150,000 specimens. The estimated value of the collection is between £40,000 and £45,000.

THE congress before adjournment passed an amendment to the food and drug act which the president has signed, making it illegal "If its package or label shall bear or contain any statement, design, or device regarding the curative or therapeutic effect of such article, or any of the ingredients or substances contained therein, which is false and fraudulent." It will be remembered that the act of 1906 declared that a drug is misbranded "the package or label of which shall bear any statement . . . which shall be false or misleading in any particular . . ."; but the supreme court by a majority of five to three decided that this did not refer to false statements regarding the curative effect of a drug.

THE congress also passed the bill carrying out the fur seals treaty, including a closed season for five years in the Pribilof Islands.

THE fifteenth International Congress on Hygiene and Demography will, as has already been announced, be held at Washington, D. C., September 23–28, 1912, with Mr. Taft as honorary president. The president of the congress is Dr. Henry P. Walcott, of Massachusetts; the secretary, Dr. John S. Fulton, Senate Annex, Washington, D. C. Thirty-two foreign countries have accepted the invitation of this government to take part. The German Medical Society of New York will hold a reception on the evening of September 18 in honor of the German and Austrian physicians who will come to this country to attend the congress.

AT the International Otological Congress held in Boston, under the presidency of Dr. Clarence J. Blake, it was decided to hold the

tenth congress at Halle, Germany, in 1915. The officers elected were: *President,* Dr. Alfred Denker, of Halle; *Vice-president,* Dr. Alexander B. Randall, of Philadelphia; *Secretary and Treasurer,* Dr. Henry O. Reik, of Baltimore.

Mr. James B. Brady, of New York, has, it is reported, given the sum of $220,000 to the Johns Hopkins Hospital, Baltimore, for the establishment of a ward for the treatment of diseases of the kidney.

The annual meeting of the Association of Military Surgeons of the United States will be held in Baltimore, from October 1 to 5, under the presidency of Surgeon Charles P. Wertenbaker, U. S. Public Health Service.

UNIVERSITY AND EDUCATIONAL NEWS

The new physics building at the University of Iowa, costing $225,000 exclusive of equipment, is now completed and will be used from the opening of the college year.

Dr. Shadworth Hollway Hodgson, the distinguished philosophical author, who died on June 3, aged eighty years, has bequeathed his philosophical laboratory to Corpus Christi College, Oxford, and his general library to Rugby School. He bequeathed £500 to each institution to defray the cost of incidental expenses.

M. E. Solvay will give $2,000 a year for three years to the Laboratory of Physical Chemistry of the Berlin University to assist the researches on which Professor Nernst is engaged.

Ira D. Cardiff, Ph.D., professor of botany in Washburn College, has resigned to accept the position of professor of plant physiology in the Washington State College at Pullman.

Dr. Sabrazès, associate professor at the laboratory of the Faculté de médecine de Bordeaux, has been appointed professor of pathology and anatomy at the same school, in place of Dr. Coyne, who has retired.

Professor Lucien Cayeux, formerly professor of general geology at the National School of Mines at Paris, has been elected to the chair of "The Natural History of Inorganic Bodies" at the Collège de France, left vacant by the death of Michel-Lévy.

DISCUSSION AND CORRESPONDENCE

A REMEDY WORSE THAN THE DISEASE

To the Editor of Science: In your issue of August 9, an article by Professor J. S. Kingsley announces various changes in the rules of zoological nomenclature proposed by certain Austrian and German zoologists, and to be submitted to the next zoological congress for approval. He inferentially asks the signatures of those interested in zoology as a backing for the proposed changes. In view of the total demoralization of zoological nomenclature which would follow the adoption of these changes (and I do not see the name of a single expert in such matters among those cited in their favor by Professor Kingsley) I feel bound to offer some comments.

I may incidentally remark that it is the past modification in a similar manner of the original British Association rules by over hasty and ill-informed action, that is responsible for ninety-nine out out of every hundred of the present difficulties. Moreover, my own experience in my own field of study leads me to believe it probable that Professor Kingsley's communication greatly exaggerates the difficulties for professional naturalists of the present state of affairs. The people who find themselves in trouble are not the men who really do modern work in systematic zoology, but are men of a past generation who are annoyed by unfamiliar names, teachers relying on out-of-date text-books, some amateurs without access to recent literature and the body of anatomists, morphologists and others, not systematists, who do not like to be bothered by nomenclature at all, but wish to get names for their material without working for them or asking some one who is by way of knowing.

I would be the last to deny that there are some real difficulties, and that it would be wise to remedy them, but the authors of this outcry have not indicated the right way to bring it about. On the contrary, in some respects it is calculated to increase the difficulties tenfold, to undo good work that is already accepted by the generality of students (for I take it for granted that the new laws are intended to be *ex post facto*), and to introduce

such a mass of uncertainty, doubt and confusion as could never be remedied. The probable result would be that most experts would refuse to accept the new system and without their acquiescence nothing good could be hoped for.

The nature of nomenclature in science is such that to be stable it must be arbitrary. Past experience as well as common sense prove that such matters of controversy left to individual opinion are never finally settled. Individuals must fall back on a general rule of action. Let us examine the proposals. We shall find them containing both good and evil. The first section of the Austrian circular is stated to restrict nomenclature to binomial works, in which I heartily concur, and which, with certain arbitrary exceptions (like Brisson), has always been the rule. The second "provides that when a species has once been removed from a genus it shall not be considered as the type of the genus in any future revision." Here we have uncertainty piled upon doubt. When is a species "removed from a genus"? What constitutes "removal"? If a species *is* the type of a genus and some one "removes" it, does it for that reason lose its character? If an error has been committed by some blundering tyro, is it to remain forever uncorrected? Here is arbitrariness with a vengeance!

The third section proposes that the decision of questions in nomenclature be taken away from experts and settled by popular vote. Anybody willing to subscribe five dollars may vote. It needs no comment.

The German Zoological Society begins with an eminently rational proposition, *i. e.*, that special cases be arbitrarily settled according to their merits by a committee of experts. In the list of examples there are few which call for dissent, though it may be remarked that *Terebratula* and *Liothyrina* are different groups, and that the species now referred to *Liothyrina* are not members of the traditional *Terebratula;* also that the entire group of students of the Brachiopoda, without a dissenting voice so far as I know, are in accord on this point. If the change be made it would in this case be solely for the benefit of those unwilling to give up a familiar blunder.

Their second proposition opens the way to chaos. Who is to decide when a given situation "will lead to lasting confusion or error"? Hardly the authors of this circular.

The third proposition returns to sanity. "Certain works are not to be considered in the determination of questions of priority." If these are settled, case by case, by expert committees, the rule is one I have long advocated; but it should not be decided by a vote of heterogeneous subscribers of five dollars. Looking over the list submitted as examples we find many of which the exclusion would probably meet with general approval; some which would probably be by expert vote retained. It should not be in any case decided without grave consideration of the effect on existing systematics.

The fourth proposition relapses into an appeal for chaos again. "Articles in encyclopedias, popular works of travel, journals of hunting and fishing, catalogues, garden journals, agricultural periodicals, political and local newspapers and other non-scientific journals which are without influence in systematic science" are to be ignored.

No one even moderately acquainted with the history of systematic zoology could make such a proposition as this, except in the haste which obscures clear thinking. All the above denounced journals which have influenced systematic science are, of course, *not* to be ignored (by the terms of the last clause of the proposal), but who cares what is done to those which have not? Systematists are only concerned with those which have; and which by the language of this self-contradictory announcement are endorsed, though not intentionally. Accepting the real intent of this proposal it seems impossible that those who propose it can have any conception of the new confusion and uncertainty it would create.

To sum up, the only practicable method of settling disputed questions of this sort is that adopted by the International Commission as now established. Each case to be decided on its merits, and decided by experts,

after proper consideration of the effect of the decision. To run with the unthinking crowd is no part of scientific business. If the present method has its drawbacks, it has also accomplished a preponderating amount of good service.

Wm. H. Dall

August 16, 1912

SCIENTIFIC BOOKS

Founders of Modern Psychology. By G. Stanley Hall. New York and London, D. Appleton & Company. 1912. Pp. ix + 471.

Of the twelve years from 1870 to 1882, the author spent nearly six as a student in Germany. The first triennium, ending with the year 1873, was devoted to philosophy, and it was at this period that I came under the influence of those men [Zeller, Lotze, Fechner and von Hartmann] characterized in the first four chapters. After coming home and teaching what I had learned from these masters and others for six years, during which my interest in more scientific methods and modes of approach grew, especially after the first edition of Wundt's "Psychologie" in 1874 and as a pupil of James and Bowditch, I passed a second triennium in Germany, to which period Wundt and Helmholtz [the subjects of the two concluding chapters] belong.

Six years in Germany, without the haunting oppression of the doctor's thesis!—such was our author's opportunity, and he made the most of what was offered. He heard Hegel from the lips of Michelet; he sat with Paulsen in Trendelenburg's seminary; he undertook work of research in Ludwig's laboratory, with von Kries as partner; he experimented with Helmholtz; he was the first American student in Wundt's newly founded laboratory of psychology; he discussed psychophysics with Fechner, the creator of psychophysics; he was present at Heidenhain's early essays in hypnotism; he attended those lavishly experimental lectures of Czermak, where hecatombs of dogs were sacrificed on the altar of science and "in one case even a horse was introduced to show heart action"; he was informed by Zöllner of the marvels wrought by Slade, and later he saw those same marvels performed "at evening parties in Berlin by a young docent in physics"; he followed courses in theology, metaphysics, logic, ethics, psychology, the philosophy of religion—in physics, chemistry, biology, physiology, anatomy, neurology, anthropology, psychiatry; he frequented clinic and seminary, laboratory and lecture; and he roamed afield as far as Paris on the west and Vienna on the east. *Non cuivis homini contingit adire Corinthum!* But Dr. Hall made the journey twice over, and took his fill of the intellectual feast.

The six men to whom the present volume is devoted have already been named. First in order stands Eduard Zeller (1814–1908), who began his public life as a protestant theologian—he married the daughter of F. C. Baur, the founder of the Tübingen school—but is better known to the present generation of scholars as the historian of Greek philosophy and the dreaded examiner at the university of Berlin, where be became professor of philosophy in 1872. Zeller is followed by Rudolf Hermann Lotze (1817–1881), the greatest name in German philosophy between Herbart and Wundt, who spent his working life in Göttingen (1844–1881) and died within a few months of his call to Berlin. Next comes Gustav Theodor Fechner (1801–1887), physicist and mystic, whose philosophy was held during his lifetime in ill repute, though its by-product brought him enduring fame as the founder of psychophysics. Fourth upon the list stands Karl Robert Eduard von Hartmann (1842–1906), the apostle of pessimism and of the unconscious, an invalid and recluse, who lived his days with philosophy and music in a cottage just outside Berlin, and who enjoyed the popularity that has fallen in later times to Haeckel and to Nietzsche. Next comes Helmholtz, unquestionably the greatest figure in the book. Last of all stands Wundt, the *Altmeister* of experimental psychology, still happily with us, though now on the eve of his eightieth birthday.

To understand the choice of these six men —for who beside the author would count Zeller and von Hartmann among the founders of modern psychology?—we must understand something of Dr. Hall's own training and temperament. Passing to Germany from a denominational American college, he took

with him an enthusiastic interest in the philosophy and psychology of religion—an interest that prompted his publication in 1872–3 of I. A. Dorner's "System of Theology" and that, persisting to the present time, has led him to establish a *Journal of Religious Psychology*, and to interpret the great philosophical systems as Freudian sublimations of religious conviction. It is significant that the names of Graf and Kuenen, to whom was due the renascence of the higher criticism in the sixties, do not appear in his pages: yet he is catholic enough to appreciate Dorner and Zeller and Delitzsch, Pfleiderer and Lazarus. So far, we may suppose, Germany continued and enriched a mode of thought which was already familiar. But there was surprise in store: the "narrow, formal, rather dry curriculum" of the college was to give way to "a great and sudden revelation of the magnitude of the field of science." And what a revelation! Those were the great days of Darwinism; the days of Haeckel, of the "Generelle Morphologie" and the "Natürliche Schöpfungsgeschichte" and the biogenetic law, of the "Descent of Man" itself! Biology was thinking in great sweeps of thought; evolution was the key to world-riddles; there was no cloud upon the horizon to warn men of the minute specialization and laborious experimentation that were to come. It is small wonder that Dr. Hall became the enthusiastic champion of a genetic psychology; and it is small wonder that his geneticism bears the indelible impress of its date of origin. This contemporary Darwinian enthusiasm is, indeed, the fount and source of most of the critical judgments passed in the book.

Along with the interest in religion and the possession by the genetic idea go two other marked characteristics: the zeal of the reformer, the exhorter, the practical educator, and a sort of perpetual youth, with an unsatiated appetite for intellectual novelties. The former is apparent throughout the work; the latter is seen in the writer's almost boyish absorption in new movements—in Freudianism, in Bergson, in the introspective departures of the Würzburg school—and crops out in the oddest personal fashion, as when one great man is censured for a stay-at-home life, and another is credited with a habit of vacation-trips. Every chapter begins in this way with a biographical sketch, which in fact makes us acquainted with the author no less than with his subject. Then follows an analysis of the subject's principal works, with more or less of running commentary and criticism; the exposition seems to be taken, in the main, from lecture-notes of the seventies and eighties, while the comment represents the writer's more mature position. Finally, the sketch ends with a general appreciation and a selected bibliography. The first five portraits in Dr. Hall's gallery occupy some sixty pages apiece; Wundt, who evidently and quite naturally has given him the greatest trouble, fills no less than a hundred and fifty.

Dr. Hall delegated to an assistant "the burden of revising and correcting the entire manuscript of the book, and seeing it through the press." It is no blame to the assistant that the slips and inconsistencies of statement, inevitable in composition of this kind, have not been removed. But in regard to what are somewhat unfairly termed printer's errors, I am afraid that blame is deserved: the avoidable mistakes of word and phrase are both numerous and grotesque.

E. B. TITCHENER

Enzymes. Six lectures under the Herter Lectureship Foundation, at the University and Bellevue Hospital Medical College. By OTTO COHNHEIM. New York, John Wiley and Sons. 1912.

This publication brings before an enlarged audience the forceful lectures upon the subject of the enzymes, which delighted those who were privileged to hear them two years ago. The book is simply written and the views therein expressed, even as regards the author's own discoveries and beliefs, are conservatively stated. It is a trustworthy guide to modern knowledge, and will be of especial value to those who have no desire to master the larger monographs on the subject. The

author gives the historical development which finally leads to the establishment of the identity of the enzymes pepsin and rennin, and suggests that curdling of milk is best explained by supposing that the curd is formed by the precipitation of a proteose of casein which is insoluble in acids and in water containing calcium salts. Cohnheim sadly but humanly remarks, "We meet here with one of the unfortunate cases in which science in stepping forward obliterates and renders useless the hard and skilful work of a whole generation of prominent men."

Of interest are the oxidative enzymes laccase and tyrosinase, which convert the aromatic cleavage products of protein into coloring matters which gradually become black. Laccase, of the lac tree, gives rise to oxyurushic acid which gives the brilliant black luster to the lacquer manufactured in Japan and China. Tyrosinase causes the production of coloring matter in the hemolymph of certain butterflies, it also attacks the proteins of the dead or dying leaves in the autumn and causes the brilliant coloring of the Indian summer; it is found in the ink-bag of the sepia; and to it may be ascribed the transformation of tyrosin into homogentisic acid in the human disease of alcaptonuria.

Written primarily for medical students, the above selections are merely an indication of the breadth of view from which the subject is surveyed.

The country should be grateful to Professor Cohnheim that, through the means of the Herter Lectureship Foundation, he has been able to add to its literature a treatise such as is "The Enzymes."

Graham Lusk

SPECIAL ARTICLES

SYMPTOMATIC DEVELOPMENT OF CANCER

So little is apparently known of the external symptoms of internal cancer in its early stages that any contribution of attentive observation would seem useful. The following personal case is fairly paralleled by another which need not be described; and the parallelism would seem to give some weight to the inferences.

During my second expedition to Seriland in the autumn of 1895 my party had occasion to climb Sierra Seri, the culminating range of the region. After leaving the wagon camp the party moved on foot (with two pack animals) over some 10 miles of gently upsloping plain to the foothills, where the real climb began; the pace taken was rather rapid and I was somewhat but not excessively tired on reaching the foothills, where the pack horses were to be sent back. Within a few minutes after starting the climb I observed a condition novel in my experience—*i. e.,* inability to lift the feet (especially the left) more than a few inches above the level at which I stood. There was no pain, scarcely any discomfort—merely the inability to raise the feet without help from the hands. Assuming it a manifestation of exhaustion, I halted the party for a time and ate lunch; but, on resuming, the condition almost immediately returned. Greatly puzzled, I abandoned the climb and started back with the Indian in charge of the pack horses, finding no difficulty in going downslope. Within fifteen minutes I was startled by a call from one of the remainder of the party making the climb, "El Gringo es muerto [The American is dead]." Even without explanation I knew this referred to W. D. Johnson, topographer of the expedition; and stimulated by the apparent tragedy I immediately turned to resume the climb to the point of the disaster—but despite the intense excitement, I had not climbed fifty steps before the former inability to lift the feet returned. So I remained in virtually helpless condition (sending my Indian up to the climbing party with specific inquiries) for perhaps half an hour; when the Indian returned with the gratifying intelligence that "El Gringo" had come to life and had gone on up the mountain—for it appeared that he had merely swooned under the stress of the long walk and the early stages of a stiff climb, and, recovering, had gone on with his accustomed persistence. This episode marked the first observed abnormality in locomotory powers which had been above the average.

The next noteworthy manifestation appeared during an expedition of 1900, when I frequently found myself unable to raise the left foot to the stirrup on mounting—indeed it became necessary generally to modify the attitude in mounting so as to permit giving a hitch upward to the left foot with the hand. Sometimes, too, on dismounting the left leg partially gave way; so that I acquired the habit of swinging out of the saddle in such a manner as to land on both feet. During subsequent months in office work I noticed an abnormal condition, though I failed at the time to associate it with that experienced in the field—*i. e.*, on rising after occupying my chair for a considerable time, either a sharp pain or a sensation of weakness was experienced in the left groin. This condition continued until the habit was acquired of rising with care and putting the weight at first wholly on the right foot.

In 1902 I noticed that the footfall sounds of my two feet as I walked the pavement were unlike; and I made considerable vain effort, sometimes with the help of friends, to find the reason for the asymmetry in movement indicated by the diversity in sound. This abnormality was not then associated with the abnormal conditions observed in field and office; but when within a year I noticed that the sole of the left shoe wore out twice as rapidly as that of the right I began to associate the several conditions, though without forming any idea as to cause.

In 1906 I suffered an epididymitis on the left; and in casting about for the cause of this attack my physician seemed so confident that it must be gonorrheal or syphilitic in origin (which I knew to be erroneous) that I gladly welcomed the occasion to have an expert blood examination made by a practitioner recently from a noted expert and school in London. The examination showed no trace of the infection suspected by the physician, but gained my confidence by detecting evidences of a typhoid fever through which I had passed some years previously; but it left the epididymitis totally unexplained.

About this time I made a trip through the Sierra Nevada region, largely in company with Gifford Pinchot, then Chief Forester, and J. A. Holmes, now Director of Mines; and in the course of the trip was much embarrassed by inability to climb or to ascend slopes of more than moderate degree—the trouble lying in the same inability to lift the feet first observed in Seriland.

In the autumn of 1909 while in field work in Washington state I noticed uncertainty in coordination of the control and movement of the left foot, especially in passing over slippery rocks or logs; and on one occasion suffered an accident of some severity due to a needless slip of the left foot. About this time also I noticed a slight bladder difficulty which continued increasingly for over a year—when treatment began for enlarged prostate. After preliminary examination and treatment of the conventional sort, prostatectomy was prescribed, and in April, 1911, I underwent the operation—which revealed a cancerous condition in which the carcinomatous tissue was of an exceptionally hard type, and too extended for complete extirpation. Recovery was tedious and complicated, and within a few weeks after leaving the hospital an epididymitis on the right developed—this time with little doubt in my mind as to the cause. The symptom of weakness and pain in the left groin also recurred with increased intensity, and a hitching gait was developed. The bladder never became completely normal; and in January, 1912, the lower intestine evidently became affected, producing assimilative difficulties of growing gravity.

In April, 1912—a year after the operation—a condition gradually developed on the tendons of the left knee similar to that first observed in the groin—the twinge of pain on sudden movement, inability to exercise full control, etc. The general burden on the system attending the abnormal development was noticed (without realization of the cause) about 1904, and increasingly thereafter.

Any significance this record may have lies

merely in bringing out the association between a series of obscure and puzzling symptoms developed in the course of several years, which finally seem to have found explanation in the cancerous growth revealed well toward the end of the series.

W J McGee[1]

REVERSIBLE CHANGES IN PERMEABILITY PRODUCED BY ELECTROLYTES

According to one opinion permeability is a relatively fixed property of the cell and is altered only as the result of injury: the alteration is then irreversible.

Another view assumes that there are reversible changes in permeability which involve no injury and which may form a normal part of the activities of the cell. If such changes occur it is clear that they may control the course of metabolism. That permeability may be altered in this manner is suggested by a number of facts,[1] but their interpretation is too doubtful to place this view on a firm basis. It is highly important that its truth or falsity be established by rigorous proof. Such proof seems to be afforded by a series of experiments, some of which are described below.

The method pursued in these experiments has been described in a previous paper.[2] It consists in cutting disks of living tissue from fronds of the common kelp (*Laminaria*) and measuring their electrical conductivity in various solutions. Under the conditions of these experiments an increase or decrease of conductivity denotes a corresponding increase or decrease of permeability.

Upon transferring the living tissue from sea water to pure sodium chloride of the same conductivity (and at the same temperature) an immediate increase of conductivity was observed. The conductivity continued to increase at a regular rate for about two hours. At the end of this time the conductivity of the tissue was equal to that of the same amount of sea water. At this point it remained stationary even when the tissue was replaced in sea water. This signifies that the tissue was dead.

In this case we are dealing with an irreversible change in permeability. It is natural to ask whether this change is not, up to a certain point, reversible. In order to test this, fresh living tissue was transferred from sea water to sodium chloride of the same conductivity (and at the same temperature); readings were then taken at intervals of two minutes. In the course of five minutes the resistance had fallen from 1,000 ohms to 850 ohms.[3] The tissue was then replaced in sea water and readings were taken at intervals of five minutes. In the course of five minutes the resistance rose to normal and remained unaltered until the following day, when the experiment was discontinued. This experiment was repeated many times under different conditions and with a variety of salts. The results were similar throughout.

In order to make certain that no injury resulted from the treatment with sodium chloride an experiment was performed to ascertain the effect of repeated treatments on the same lot of tissue. In one experiment the tissue was treated with sodium chloride until the resistance dropped from 1,020 ohms to 890 ohms and was then replaced in sea water, after which the resistance rose to 1,020 ohms; this was repeated daily on the same lot of tissue for fifteen days. On the tenth day the tissue began to show a falling off in resistance, which continued to the fifteenth day, when the experiment was discontinued. As this falling off was also shown by the control, which was kept in sea water throughout the experiment, it was not due to the sodium chloride, but to other causes.

The objection may be made that in this experiment the increase in conductivity was due to an increase in the number of sodium ions and that these may normally penetrate the cell more easily than the other ions of the sea water: it might therefore be unnecessary

[1] Dr. McGee died on September 5, 1912.—*Editor.*

[1] For a recent summary see Höber, "Physikalische Chemie der Zelle und Gewebe," Kap. 7 und 10, Dritte Auflage, 1911.

[2] Science, N. S., XXXV., p. 112, 1912.

[3] All the figures in this paper refer to readings taken at 18° C.

to assume any alteration in the normal permeability of the protoplasm. This supposition can not be correct, for experiments showed that the other ions of the sea water penetrate with about the same rapidity as those of sodium chloride, but in order to be absolutely sure of this point it was tested by employing in place of sea water a solution composed of 1,000 c.c. NaCl .52M plus 20 c.c. $CaCl_2$ .278M. In this solution the proportion of sodium ions to calcium ions is about 100 to 1, as is evident from the fact that both the .52M NaCl solution and the .278M $CaCl_2$ solution have the same conductivity (which is the same as that of the sea water). In this solution the conductivity of the tissue is about the same as in sea water. If we now transfer to NaCl .52M there will be an increase of about 2 per cent. in the number of sodium ions. Consequently (on the supposition that sodium ions penetrate more easily than the other ions of sea water) we may not expect an increase of more than 2 per cent. in the conductivity as long as the permeability remains unaltered. But if the increase is more than 2 per cent. it signifies a corresponding increase in permeability.

In the mixture of 1,000 c.c. NaCl .52M plus 20 c.c. $CaCl_2$ .278M the tissue was found to have a resistance of 1,020 ohms: after two hours the resistance was unaltered. The material was then transferred to NaCl .52M and left until the resistance fell to 860 ohms. It was then replaced in the mixture of NaCl and $CaCl_2$: the resistance soon rose to 1,020 ohms and remained unaltered for several hours; it was left in the mixture over night and on the following morning the resistance was still the same. It subsequently remained the same as that of the control which was kept in sea water throughout the experiment.

In order to find out how much of the resistance is due to living protoplasm the tissue was killed by exposing it for ten minutes to 2 per cent. formalin in sea water;[4] the resistance fell to 320 ohms. On subtracting this from the resistance observed when the tissue is alive we obtain approximately the resistance due to the living protoplasm; this may be called for convenience the *net resistance*, while the resistance before the subtraction is made may be called the *gross resistance*. The net resistance in this experiment was accordingly $1{,}020 - 320 = 700$ ohms before treatment with NaCl and $860 - 320 = 540$ ohms after treatment with NaCl; the net conductance before treatment with NaCl was therefore $1 \div 700 = .001428$ mho. and after treatment with NaCl $1 \div 540 = .001852$, a gain of 29.7 per cent. (the gain in gross conductance was 18.6 per cent.).[5]

It is therefore evident that there has been a very marked increase in permeability which is completely reversible.

Electrolytes may also cause a reversible decrease in permeability. The simplest way of demonstrating this is by means of the following very striking experiment. The resistance of a cylinder of living tissue in sea water was found to be 750 ohms. It was tested an hour later and found to be the same. Sufficient lanthanum nitrate was then added in solid form to make its concentration[6] in the sea water about .01M. After five minutes the resistance rose to 900 ohms. As the conductance of the dead tissue was found (at the end of the experiment) to be 315 ohms, the net resistance before the addition of lanthanum was $750 - 315 = 435$ ohms and the net conductance $1 \div 435 = .0023$ mho. After treatment with lanthanum nitrate the net resistance was $900 - 315 = 585$ ohms and net conductance $1 \div 585 = .001709$, a loss of 25.6 per cent.

[4] Check experiments showed that killing in this way has the same effect on the resistance of the tissue as killing it by means of heat or by iodine vapor or by allowing it to die a natural death.

[5] Owing to the fact that the cylinder of tissue was of the same size in each set of experiments a calculation of the specific resistivity and of the specific conductivity was unnecessary.

[6] The concentration was reduced by the precipitation of a small amount of lanthanum sulphate: this had practically no influence on the subsequent result, since the outcome is the same if we use in place of sea water a mixture of 1,000 c.c. $NaCl \cdot 52M + 20$ c.c. $CaCl_2 \cdot 278M$, in which case no precipitate is formed. It should be noted that the addition of lanthanum chloride has the same effect as the addition of lanthanum nitrate.

In order to ascertain whether this change in permeability is reversible the tissue was replaced in sea water. In the course of an hour its resistance returned again to the original condition.[1] The experiment was then repeated three times on the same lot of material with the same result; it was then allowed to stand over night in sea water. On the following day there was no appearance of injury and its resistance was the same as that of the control which had remained in sea water throughout the experiment. The tissue was then placed in the sea water plus lanthanum and left until its resistance had increased about 150 ohms; it was then put back into sea water and left until the resistance fell to normal. This was repeated three times and the tissue was then allowed to stand over night in sea water. On the third, fourth and fifth days the same experiment was repeated four times. On the fifth day the tissue appeared to be in as good condition as the control and had a resistance which was slightly higher. There was therefore no reason to suspect that the changes in permeability had been attended by any injurious effect.

Similar experiments were performed in which calcium chloride was used in place of lanthanum nitrate. In this case 3.3 gm. anhydrous $CaCl_2$ were added to each 1,000 c.c. of sea water. Owing to the fact that the rise in resistance took place more slowly than when lanthanum was used, the experiment was performed twice on each of the five successive days. On the sixth day the material was in as good condition as the control and had the same resistance.

It is therefore evident that the permeability may be greatly decreased and then restored to the normal several times on five successive days without any trace of injury. Further experiments showed that the permeability may be alternately increased and decreased twice daily for five days without injury. The amount of increase and decrease was about the same as in the experiments just described.

[1] If the material is left in sea water plus lanthanum nitrate the increased resistance is maintained for a long time unaltered.

Experiments on dead tissue (killed by heat or by formalin or allowed to die a natural death) showed that the results described above are due entirely to the living cells.

A very marked decrease of permeability may be produced by a considerable variety of other salts.

The addition of these salts in solid form simultaneously increases the conductivity of the solution and decreases the conductivity of the tissue. This affords the most convincing proof that the change in the conductivity of the tissue in these experiments can not be due to any cause other than a change in permeability; for the concentration of the ions of the sea water remains unchanged, and if they were able to penetrate as freely as they did before the addition of the salt the resistance would not increase. It would, in fact, diminish on account of the increased conductivity of the solution held in the cell walls, as is clearly shown by experiments on dead tissue.

It may be remarked incidentally that these experiments effectually dispose of the possible objection that the current passes between the cells but not through them. Were this objection well founded the decrease in conductivity could be explained only as the result of a decrease in the size of the spaces between the cells. This decrease could not be brought about except by greatly reducing the thickness of the cell walls. Both macroscopic and microscopic measurements show most conclusively that this does not occur. The contrary effect would be produced by the addition of salts in solid form, for they would tend to produce plasmolysis and thereby to increase the space between the cells.

Results.—1. It is possible to cause rapid and very large changes in permeability by means of electrolytes.

2. These changes may consist in either an increase or a decrease in permeability.

3. Within wide limits these changes may be completely reversible and entirely devoid of injurious effects.

W. J. V. Osterhout

Laboratory of Plant Physiology,
Harvard University